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IS THE ARRANGEMENT OF THE GENES IN THE CHROMOSOME LINEAR?

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Every biologist is familiar with the remarkable discoveries of Morgan and his associates concerning the germ-cells of Drosophila. One of the most important of these discoveries is concerned with the phenomenon of linked inheritance. This kind of inheritance, while entirely conformable with Mendel's law, forms a very distinct and important class of cases whose existence has been brought to light since the rediscovery of the general law in 1900. Under the general law it is found that characters which behave as distinct units in heredity assort quite independently of each other. Thus if parents are crossed one of which possesses two characters, A and B, while the other lacks them, then the offspring of this cross will transmit A and B sometimes associated in the same gamete, sometimes in different gametes, the two events being under the laws of chance equally probable.

But in linked inheritance, a phenomenon first made known to us through the work of Bateson and his associates in England, later more fully explored and explained by Morgan, A and B are not wholly independent of each other in transmission. If they enter a cross together, they have a tendency to stay together in later generations; and if they enter a cross separately, they have a tendency to remain apart in later generations. Morgan has suggested that what binds or links two characters together is the fact that their genes lie in the same body within the cell-nucleus. Such bodies he supposes are the chromosomes. The evidence for this conclusion is very strong. Morgan and his associates have demonstrated the existence in Drosophila of four groups of linked genes corresponding with the four pairs of chromosomes which the cell-nucleus of Drosophila contains. Morgan has further suggested (and has beyond doubt established the fact) that the genes within a linkage system have a very definite and constant relation to each other. He supposes their

arrangement to be linear and in the group of genes most exhaustively studied, that of the 'sex chromosome' has represented them in a 'chromosome map, as shown in Diagram I.

That the arrangement of the genes within a linkage system is strictly linear seems for a variety of reasons doubtful. It is doubtful, for example, whether an elaborate organic molecule ever has a simple string-like form. Let us, therefore, examine briefly the evidence for or against the idea of linear arrangement of the genes. It is supposed by Morgan that two genes lying in the same chromosome show close linkage if they lie close together, but less linkage if they lie farther apart, and that the farther apart they are the less will be their linkage. As a measure of the distance apart of two genes he takes the percentage of cross-overs between them. This term requires a word of explanation. If two genes, A and B, enter a cross in the same gamete and emerge from it in different gametes, one of them has evidently *crossed-over* from the chromosome in which it lay to the mate of that chromosome (all chromosomes being paired prior to the formation of gametes). Likewise if the two genes, A and B, having entered a cross separately (being contributed by different parents), later emerge from the cross together, it is evident that one of them has again *crossed-over* so as to lie in the same chromosome as the other. The readiness with which cross-overs occur between two genes will on Morgan's hypothesis depend on their distance apart and the percentage of cross-overs between genes will be proportional to the distances between them. These assumptions have abundantly proved their utility as a working hypothesis, for it has been found possible, knowing what certain cross-over values are, to predict others with a fairly good degree of accuracy.

If the arrangement of the genes is strictly linear, so that A, B, C, etc., lie in a straight line, then it should be possible to infer the distance AC, if the distances AB and BC are known, since AC = AB + BC. But if the distance AC is less than the sum of AB and BC, then the arrangement can not be linear, since B will lie out of line with A and C. In reality it has been found that the distances experimentally determined between genes remote from each other are in general less than the distances calculated by summation of supposedly intermediate distances, and the discrepancy increases with increase in the number of known intermediate genes. To account for this discrepancy Morgan has adopted certain subsidiary hypotheses, of 'interference,' 'double crossing over,' etc., in accordance with which it is supposed that cross-overs between nearer genes interfere with or lessen the apparent amount of crossing-over with genes more remote. He therefore bases his chromosome map on summation of the shorter distances. This, however, leads to results which can be shown to be impossible.

Morgan's map of the sex-chromosomes places five out of twenty-nine genes at distances between 55 and 66 from the zero end of the chromosome, where yellow is located. A moment's reflection will show these to be impossible re-

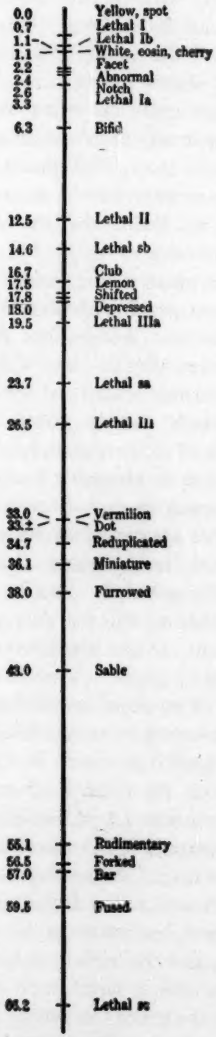

DIAGRAM I.

"Diagram I shows the relative positions of the genes of the sex-linked characters of *Drosophila*. One unit of distance corresponds to 1% of crossing-over."

Morgan and Bridges, p. 23.

lations, for a cross-over percentage greater than fifty is absurd. If A and B assort wholly independently, without any linkage whatever, just as they would in ordinary Mendelian inheritance where no linkage exists, cross-overs and non-cross-overs will be equal, 50% each. Any cross-over value consistently less than 50% shows linkage. *A cross-over value greater than fifty cannot exist.* For there must be either linkage or no-linkage. But no-linkage means 50% cross-overs, and linkage means less than 50% cross-overs. Hence a value greater than 50% cannot occur.[1] As an alternative to the hypothesis of linear arrangement it is possible to suppose that the arrangement of the genes is not linear, that the nearer genes are not directly in line with the more remote ones.

If the arrangement of the genes is not linear, what then is its character? This query led me to attempt graphic presentation of the relationships indicated by the data of Morgan and Bridges[2] but finding this method unsatisfactory I resorted to reconstruction in three dimensions, which has proved very satisfactory. The data used consist of the experimentally determined cross-over percentages between twenty genes of the sex-chromosome of Drosophila, as given in Table 65 of Morgan and Bridges. The only hypothesis involved in the reconstruction is Morgan's fundamental one that distances between genes are proportional to cross-over percentages. The secondary hypothesis, that distant genes are really farther apart than indicated by the experimental data, is rejected for the reason already explained, that impossible relationship are thereby entailed. Taking the data, then, exactly as they stand, we find it possible to make a very complete and on the whole self-consistent reconstruction of the architecture of the sex-chromosome from the linkage relations of its genes. A small ring of wire is taken to represent the locus of a gene. Two genes are placed as far apart (in inches) as there are units in the cross-over percentage between them. Thus between yellow-body and white-eye there is shown by Morgan's data to be a cross-over value of 1.1%. Consequently the rings which represent these genes in the reconstruction are joined by a wire 1.1 inches long. Between white and vermilion the cross-over percentage is 30.5. Accordingly the connecting wire in this case is made 30.5 inches long. Proceeding in this way the reconstruction shown in figures 1 and 2 is obtained. It indicates an arrangement of the genes not by any means linear, but rather in the form of a roughly crescentic plate longer than it is wide, and wider than it is thick. It is shown in side view in figure 1, and in edge view in figure 2.

That the arrangement of the genes can not by any possibility be linear is shown by two critical cases, the loci for *bifid* and *abnormal*. Bifid (*Bi*, figs. 1 and 2) is shown by a series of over 3,600 observations to be at a distance 5.5 from yellow. It is almost the same distance from white, viz., 5.3, as shown by 23,595 observations. Yet white and yellow are distant from each other only 1.1 units, as shown by 81,299 observations. Therefore bifid can lie neither above nor below yellow and white, in the line which joins them, but

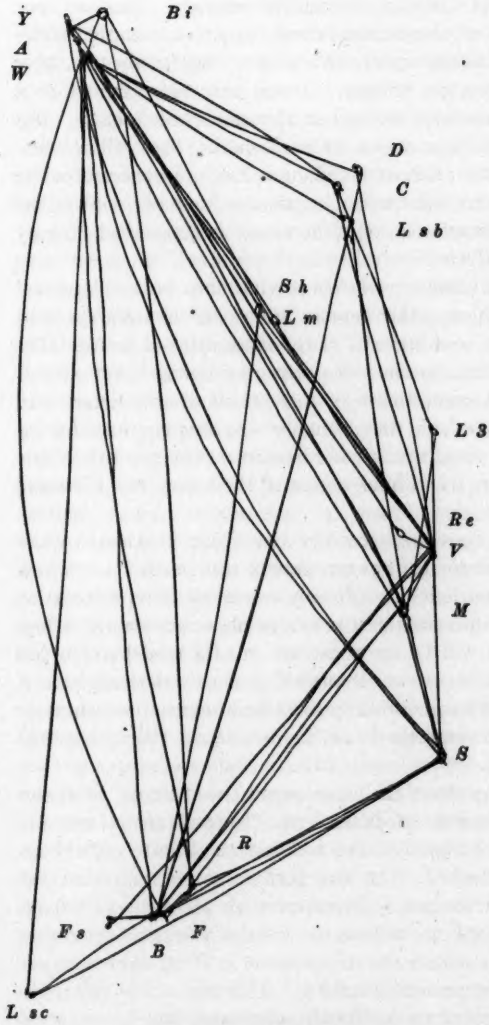

FIG. 1. SIDE VIEW OF MODEL

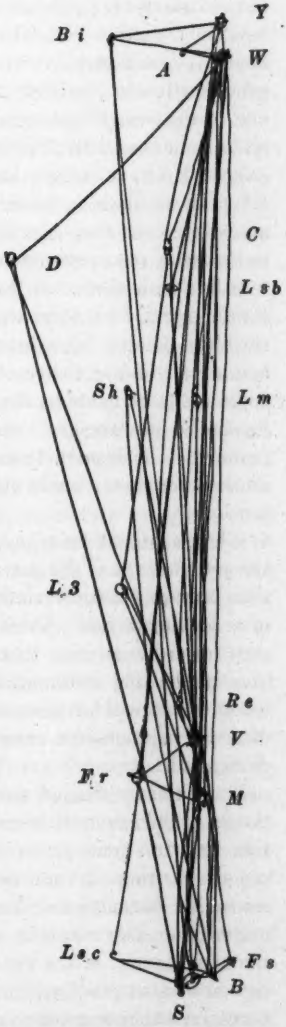

FIG. 2. EDGE VIEW OF MODEL

Seen at right angles to figure 1.

Showing relative positions of genes of 20 sex-linked characters of Drosophila, linear arrangement not being assumed. For significance of letters, compare Diagram I.

must lie laterally about equidistant from both. Other linkage relationships of bifid show that it lies as far as possible removed from the plane in which most of the genes lie. The other case mentioned is that of abnormal abdomen (*A*, figs. 1 and 2). Its linkage relations are known with only two other genes, yellow and white. But the relation of these two with each other is one of the best determined and the linkage of abnormal with each of them rests on more than 15,000 observations in each case. The yellow-white linkage is 1.1, as already stated; abnormal-yellow is 2.0, and abnormal-white 1.7. These relations make it impossible for abnormal to lie in line with yellow and white. Until a third linkage relation of abnormal is determined, it may swing freely round the line which joins yellow with white but can never come into line with them. A third linkage relation having once been determined for abnormal, its linkage with any other gene in the sex-chromosome could be readily predicted from direct measurement of the reconstructed figure. The actual test of the utility of this method of portraying linkage relationships could easily be made by first forecasting by measurement what undetermined linkage values are likely to be and then actually determining them by experiment. Such predictions could not fail to come nearer the truth than predictions based on a linear map, if as I have suggested the arrangement is really not linear.

What, it might be asked, does this reconstruction signify? Does it show the actual shape of the chromosome, or at any rate of that part of it in which the observed genetic variations lie? Or is it only a symbolical representation of molecular forces? These questions we can not at present answer. A first step toward answering them will be the construction of a model which will give us reliable information as to undetermined genetic relationships. A model which will answer questions truthfully must be a truthful presentation of actual relationships even though we do not know whether they are spatial or dynamic.

If the arrangement of the genes in the chromosome is not linear, Morgan's theory of linkage must be somewhat modified. (1) The fundamental assumption that the genes lie in the chromosomes and have a definite orderly arrangement there is not disturbed. (2) The further assumption that the respective distances between the genes determine their closeness of linkage one with another may also stand unchallenged. (3) But the assumption that the arrangement of the genes within the chromosome is linear cannot be accepted without proof, which at present is lacking. This assumption has made necessary other secondary assumptions, likewise unproved, which are superfluous if this one is abandoned.

Such an unproved secondary hypothesis is that of double-crossing-over. The experimental data show that double-crossing-over *must occur*, if the arrangement of the genes is linear. For if three genes, A, B, C, are linear in their arrangement in the order named, and all lie in the same gamete, and if subsequently A and C are found together in one gamete and B in another,

it is evident that this rearrangement can have come about only as a result of two breaks in the linkage chain, viz., one between A and B, and another between B and C. But if the arrangement is *not* linear, double-crossing-over need not be assumed as an explanation of the observed regroupings. For if A, B, and C are linked in a triangle, not in a straight line, then B may be freed from its connections with A and C without necessarily disturbing the connection of A and C with each other. Freeing of B will involve no greater number of breaks than the freeing of either A or C. It will still be true, however, as indicated by the experimental data, that certain groupings of three particular genes are easier to obtain than others. Thus in the case of the three genes white, bifid and vermilion, it is hardest to obtain the regrouping which involves detaching bifid from the other two. Morgan assumes that this is because bifid lies *between* the other two in a single linkage chain and so could be detached only by two breaks; it is possible, however, that the reason may be that bifid lies peripherally in the linkage system and could be detached only by an oblique longitudinal break, whereas either of the others could be detached by a simple transverse break. Similarly in the trio, white-vermilion-sable, it is vermilion which is difficult to detach; and in the group, vermilion-sable-bar, it is sable. Always it is the *middle one* considered with reference to the long axis of the system. This may be because, as Morgan supposes, only transverse breaks occur, of which two taking place simultaneously are required to produce the difficult regrouping, or it may be because transverse breaks are more frequent than oblique longitudinal ones, of which a single one would suffice to accomplish the regrouping, if the genes are not strictly linear in arrangement.

The phenomenon of 'coincidence' as described by Weinstein[8] is this. If crossing-over occurs toward one end of a chromosome, it is less likely to occur simultaneously elsewhere in the same chromosome. Crossing-over in one part of a chromosome is thus supposed to 'interfere with' crossing-over elsewhere in the same chromosome. If we adopt the hypothesis of linear arrangement, interference must be assumed to occur. Observed facts require this. But if we do not adopt this hypothesis but suppose that what have been called 'double cross-overs' are really the result of single oblique or single longitudinal breaks, then the supposed phenomenon of interference may mean only this, that transverse breaks are more likely to occur than longitudinal ones.

Finally, if the genes are not arranged in a single linear chain, the chiasma-type theory will need to be reëxamined. Such a purely mechanical theory seems inadequate to account for interchange of equivalent parts between twin organic molecules, such as the duplex linkage systems of a germ-cell at the reduction division must be. It seems more probable that preceding the reduction division a period of instability within the chromosome molecule comes on. Twin molecules are now closely approximated and parts of one may leave their former connections and acquire new connections with the

corresponding parts of the other twin. It is evident from the experimental data, notably that of Muller,[4] that new connections are not formed with any torn fragment of chromosome which happens to come into the proper position, but that connections are always formed at exactly corresponding points with homologous systems of genes.[5] It is like the replacement of one chemical radicle with another within a complex organic molecule and it seems highly probable that such is its real nature.

[1] The distances shown in Morgan's chromosome map in excess of 50 (admittedly not obtained experimentally but only by summation) are therefore too large. Accordingly, if one clings to the assumption that the arrangement of the genes is linear, it must be, not that the longer distances are too short, as Morgan has assumed, but that the short distances are too long. Therefore, any hypotheses framed to account for an apparent shortening of the long distances are superfluous. The long distances given by direct experiment are long enough; they approach the limit of the possible, viz., 50%. Thus in table 65 of Morgan and Bridges, we find the following high cross-over percentages given by direct experiment:—yellow-bar, 47.9; white forked, 45.7; and white-lethal *sc*, 46.0. What is needed therefore, if the linear arrangement hypothesis is retained, is a secondary hypothesis to explain why the short distances given by experiment are too long.

But if we abandon the hypothesis of linear arrangement, all secondary hypotheses are unnecessary. The experimentally obtained cross-over percentages may be accepted at their face value, which in every case fall within the limits of the possible, 0 and 50.

[2] Morgan, T. H., and Bridges, C. B., Sex-linked inheritance in Drosophila. *Carnegie Inst. Washington, Publ.*, No. 237, 1916, (88 pp., 2 pl.).

[3] Weinstein, A., *Genetics*, 3, 1918, (135–172).

[4] Muller, H. J., *Amer. Nat., Lancaster, Pa.*, 50, 1916.

[5] The case of 'deficiency' studied by Bridges (*Genetics*, 2, pp. 445–460, Sept. 1917) forms an apparent exception to the rule. Here a certain segment of the linkage system was as regularly wanting as it is commonly present. The regularity of the process, however, shows that the principle of union at particular points still holds. In the deficiency race, a new and simplified linkage system had been established and this persisted.

THE LINKAGE SYSTEM OF EIGHT SEX-LINKED CHARACTERS OF DROSOPHILA VIRILIS (DATA OF METZ[1])

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In an earlier paper it has been shown that the arrangement of the genes in the sex-chromosome of *Drosophila ampelophila* is probably not linear, and a method has been developed for constructing a model of the experimentally determined linkage relationships. From such a model one may by direct measurement ascertain what other undetermined linkage values are likely to be. In order to test the utility of this method, it is desirable that it be tried out as widely as possible and the results for different cases compared with each other. For such use, suitable material is found in a paper by Metz[1] dealing

with the linkage relations of eight sex-linked characters in *Drosophila virilis*, a species distinct from *D. ampelophila*, which has been so exhaustively studied by Morgan and his pupils. Of the eight characters studied by Metz, two agree morphologically and in their linkage relations with each other, with similar characters of *D. ampelophila*. The six others have no exact counterpart among the known mutations of *D. ampelophila*. The two characters in question are yellow body and forked bristles. Yellow body, in both species, lies at the extreme, 'zero' end of the linkage system. Forked, in both species lies at a distance of 40 or over from yellow. In *D. virilis* the distance is exactly 40, according to the observations of Metz; but in *D. ampelophila*, according to Morgan and Bridges, the distance is about 6 or 7 units greater. But inspection of figure 1 (p. 29) shows that this estimate is probably too

TABLE 1

GENES	TOTALS	CROSS-OVERS	PERCENTAGE CROSS-OVERS
Yellow-frayed	308	4	1.3
Yellow-vesiculated	3,548	621	17.4
Yellow-magenta	3,049	1,157	38.0
Yellow-hairy	162	65	40.1
Yellow-forked	3,781	1,510	40.0
Yellow-glazed	1,328	591	44.5
Yellow-rugose	1,060	502	47.3
Frayed-vesiculated	296	55	18.6
Vesiculated-magenta	2,799	944	33.7
Vesiculated-forked	3,761	1,395	37.1
Vesiculated-glazed	877	359	42.1
Vesiculated-rugose	1,696	729	42.9
Magenta-forked	2,529	95	3.7
Magenta-rugose	824	197	23.9
Hairy-forked	162	5	3.1
Forked-glazed	749	193	25.7
Forked-rugose	1,158	321	27.7

high, since the wire joining white (*W*) with forked (*F*) in the model is too long to harmonize fully with other linkage-values given by Morgan and Bridges, the wire being curved. The distance yellow-forked is not given by Morgan and Bridges but it evidently should be about one unit greater than the distance, white-forked, which is given as 45.7. If this estimate of the distance is too high, as figure 1 indicates, then the distance yellow-forked is probably not very different in the two species of Drosophila and will be found to be not far from 40 in both.

The linkage values found by Metz for the eight sex-linked genes of *D. virilis* have been gathered from his several tables, averaged and brought together in table 1 herewith. They form the basis of the reconstruction shown in figures 3 and 4.

Metz, adopting Morgan's system of linear grouping, shows the eight genes in a linear chain more than 80 units long, although the greatest distance experimentally found between any two genes is 47.3 (yellow-rugose). This discrepancy, like the similar ones observed in *D. ampelophila*, shows the inadequacy of the hypothesis of linear arrangement. For the maximum possible

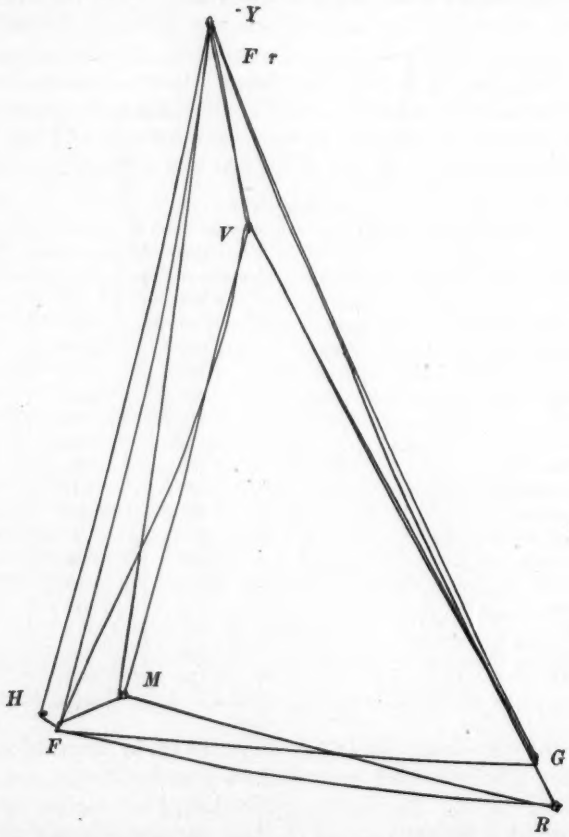

FIG. 3. MODEL SHOWING RELATIVE POSITIONS OF GENES OF 8 SEX-LINKED CHARACTERS OF DROSOPHILA VIRILIS

cross-over percentage is 50 and this is in no case exceeded by data given by direct experiment.

Reconstruction in three dimensions (figs. 3 and 4) shows even more clearly in this case than in that of *D. ampelophila*, that a linear arrangement is out of the question. The reconstructed figure is roughly in the form of a tetrahedron. Figures 3 and 4 are views taken at right angles to each other corre-

sponding with the two views given for *D. ampelophila* in figures 1 and 2. The eight genes lie in four groups at the four apices of the figure, in groups of 2, 3, 2, and 1 respectively. The figure has a very definite and rigid form, for geometrical reasons. Three additional linkage relations should be known to determine fully the position of certain of the genes within their respective

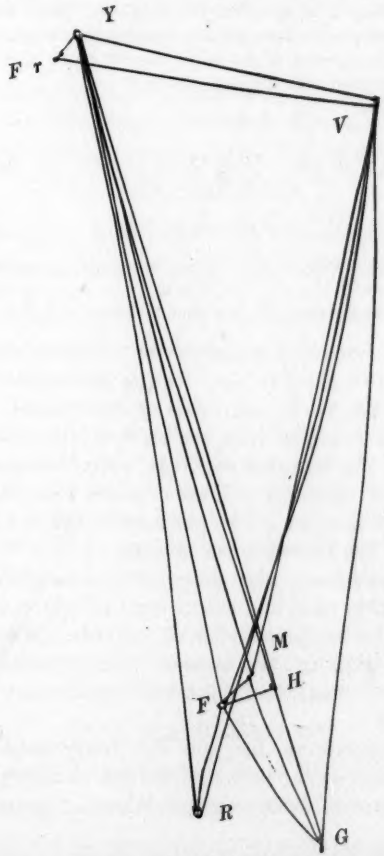

FIG. 4. MODEL SEEN AT RIGHT ANGLES TO FIGURE 3

groups. These are magenta-hairy, glazed-rugose, and frayed-forked (or frayed-glazed), where the figures show connecting wires to be wanting.[2] Given these missing connections, the form of the model would be very fully determined in every detail. Any newly discovered sex-linked gene of *D. virilis* having been located with reference to three of these eight genes, its rela-

tions to all the others could be predicted by direct measurement from the model.

[1] Metz, C. W., *Genetics*, 3, 1918, (107–134).

[2] From measurement of the model, it may be predicted that the cross-over percentage between magenta and hairy will be about 4 or 5, and the glazed-rugose percentage about the same, probably a little greater. The position of frayed in the system is not fully determined, as only two linkage relations of frayed are known. But it may be predicted, from measurement, that the frayed-forked cross-over percentage will lie between 39 and 41, and that frayed-glazed will lie between 43 and 46, provided of course that the relations given in table 1 have been determined with sufficient accuracy.

THE CAUSE OF PROLIFERATION IN BEGONIA PHYLLOMANIACA

By Erwin F. Smith

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Read before the Academy, November 18, 1918

The cause of the excessive production of adventive shoots on the leaves and internodes of this plant (a very strange phenomenon) is attributed to excessive loss of water, due to woundings or other causes. Usually in regeneration the response is not far from the place of injury, here it may be at a long distance from the wounded part, e.g., roots wounded and response in the top of the plant, although a direct response from the injured part can also be obtained. The paper will be published in full in *The Journal of Agricultural Research*. The following is a synopsis:

1. Ordinary begonia leaves when detached from the plant and pegged down on moist sand develop roots and shoots from cut places and this method is used by gardeners for the propagation of begonias. Many other plants are propagated in this way, e.g., the hyacinth from bulb scales.

2. But the leaves and shoots of this begonia *proliferate while still attached to the plant.*

3. They will proliferate on the plant very freely when wounded, making small forests of shoots on the thickened red lips of the wound if the wounds are made in quite young tissues, but not otherwise (young leaf blades were used).

4. They will frequently proliferate in the top parts of cuttings (on leaves and internodes) especially if the cuttings are dried for a day or two before planting.

5. They will proliferate most astonishingly at the top of the plant (both from leaves and internodes) if the roots are wounded, but here again only quited young tissues can be shocked into the production of such shoots. This is the most striking fact I have discovered, viz., that the prolification may occur at a long distance from the place of wounding and must be from young

tissues. So far as known to the writer, it is the first example of response of this sort at a distance from the point of injury.

6. I have also some evidence that leaves will proliferate locally under colonies of sucking insects (mealy bug, white fly), also that withholding water from the plant for a few days will cause it to proliferate.

7. The nature of the shock appears to lie in the sudden interruption of the water current which is conceived to cause cell-precipitates or plasmolysis of young totipotent cells which begin to grow when they have recovered from the shock.

8. The prolification at times is so much like a forest that one must assume that the whole surface (epidermis) of immature shoots is full of cells capable of growing into new plants if properly shocked but that as the tissue matures these cells either lose their power of response, or become more perfectly protected.

9. These adventive shoots, for the most part, perish quickly and cannot be regarded as branches, since they have no initial connection with the ordinary cambium, or xylem-phloem of the mother plant. They are rather to be classed with filial teratomas. Later, a small proportion of them establish connections with the conductive tissues of the mother and persist, i.e., become abnormally situated branches.

10. My observations contradict those of Prillieux and confirm those of Verlot and of Caruel that buds may arise from the ordinary trichomes. They may develop either from the base or the middle of acicular hairs. Such hairs arise from a red tissue, the other parts of the epidermis being green. I have also seen them developing from the base of glandular hairs which are abundant on the young internodes, but they are not restricted to these pairs.

THE PERCENTAGE NUMBER OF METEORITE FALLS AND FINDS CONSIDERED WITH REFERENCE TO THEIR VARYING BASICITY

By George P. Merrill

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Communicated by C. G. Abbot, January 9, 1919

Various compilations relating to time and distribution of meteorite falls have been made with a view of correlating them with periodic showers, but with, thus far, the only result of showing that there is no apparent connection between them.[1] Viewing the subject from a geological standpoint, that is, from the standpoint of an earth made up by the gradual accumulation of meteoric materials, and considering also the apparent more basic nature of the earth's interior as compared with the outer portion, I have thought it possible some light might be thrown upon it through a consideration of the

percentages of actually observed falls and the relative basicity of their materials. The results of such consideration are given below:[2]

Of the total 367 known meteoric irons there were seen to fall but 17, or less than 5%. These are essentially metallic; ultra basic.

Of the 31 known stony irons variously classed as Lodhranites, Pallasites and Mesosiderites, carrying at times as high as 50% metal, there were seen to fall but 5, or in round numbers 16%.

Of the 370 known stones composed mainly of silicate minerals, with chondritic structure, carrying from 5 to 25% metal (Howarditic chondrites to Ureilites inclusive), there were seen to fall 322, or 87%.

Of the 21 calcium-aluminum-rich stones, carrying less than 1% metal, free of chondrules, and variously classed as Angrites, Eukrites, Shergottites and Howardites, there were seen to fall 20, or 95%.

Of the 12 magnesia rich stones essentially free from metal without chondrules and classed as Bustites, Chassignites, Chladnites and Amphoterites, the most acidic types known, there were seen to fall 12, or 100%.

As there is little reason for supposing that falls of one kind are not as conspicuous as those of another, it would seem a fair assumption that those of which the seen percentage was the smallest were the earliest, perhaps largely prehistoric.[3] Hence arises the thought of a gradual decreasing basicity or what is the same thing, increasing acidity of accumulated materials, as time goes on. While it would seem absurd to claim that such a change could manifest itself perceptibly during the few years of observation, it is nevertheless worthy of note that, however much uncertainty is attached to the period of fall of upwards of 95% of the known meteoric irons, the stones of the last two classes mentioned, which are of the most acidic type and with one or two exceptions are iron free, have fallen within a period of a little more than one hundred years.

Following out the same line of thought, it would seem possible that the thousands of meteors which are known to enter our atmosphere daily and yet leave no record of their fall, might be products of a still further differentiation of cosmic matter (or perhaps derived from an entirely different source) and of such eminently combustible material as to be largely consumed in their flight.

Additional interest is attached to this suggestion from the fact that there are known but eight carbonaceous meteorites, i.e., eight stones in which an uncombined carbon (or possibly hydrocarbon) occurs in such quantities as to give them a distinctive character and which, therefore, might be considered liable to destruction by heat while passing through our atmosphere. All of these eight were seen to fall, the first, that of Alais, France, in 1834 and the latest, that of Indarch, India, in 1891.

It is possible to account for some of the facts here given on the assumption that many meteorites are of an extremely perishable nature, and unless seen to fall and sought for immediately, likely to become destroyed through dis-

integration. Further than this, a meteoric stone would be less likely to attract the attention and curiosity of the ordinary individual than would an iron. So far as the first possibility is concerned, I think that all who have had to do with meteorite collections will agree that as a general rule the irons, through their susceptibility to a damp atmosphere and consequent rusting, require much more attention for their preservation than do the stones. The second possibility is, however, one that must be given consideration.

[1] See Chapter IV of Farrington's *Meteorites*, Chicago, 1915.

[2] The figures here given relative to number of falls are believed to be substantially correct up to 1916. Accurate statistics since that date are not available.

[3] It would be a natural supposition that the fall of an iron would be less noticeable than that of a stone since the former would be less liable to break up—explode—in its passage through the atmosphere. Unfortunately, the literature is not sufficiently explicit on this point to bear out the supposition. Hidden, to be sure, states that the fall of the Mazapil iron was accompanied only by a loud sizzing sound, there being no explosion or loud detonation. On the other hand, Kunz states that the fall of the Cabin Creek iron was "accompanied by a very loud report which caused the dishes to rattle," and the fall of the Nedagolla iron is also stated to have been accompanied by an explosion. Accounts of other falls are either noncommittal on this point or equally contradictory, and it is evident accurate information is lacking.

NOTE ON A CONTACT LEVER, USING ACHROMATIC DISPLACEMENT FINGERS

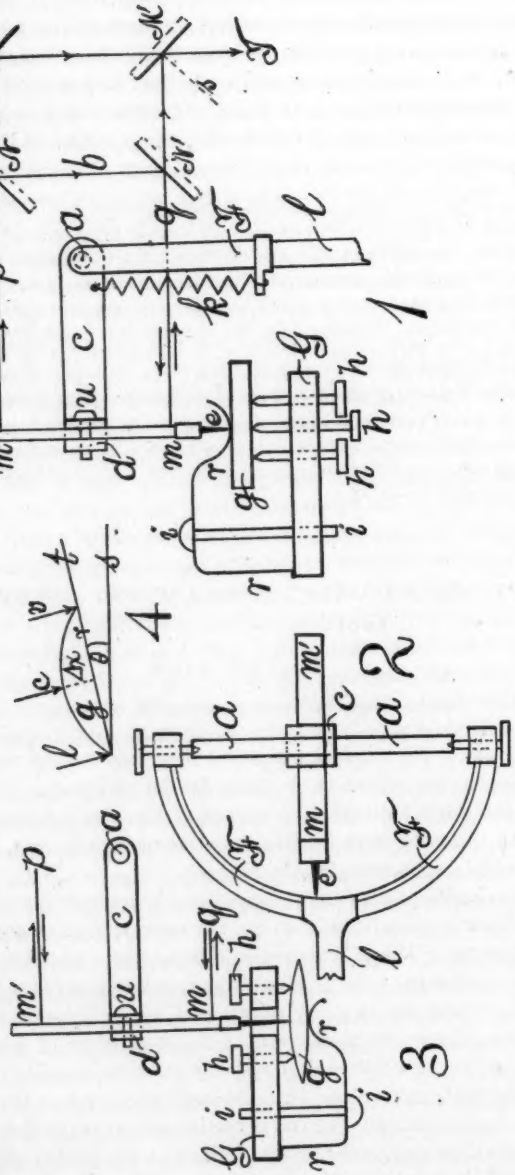
By Carl Barus

Communicated, December 27, 1918

1. Apparatus.—The method heretofore described for the measurement of small angles by the aid of the rectangular interferometer, lends itself conveniently for the construction of apparatus like the contact lever, or the spherometer. Having work needing such instruments in view, I designed the following simple apparatus for the purpose.

Figure 1 is a plan of the design; figure 2 an elevation of the fork and appurtenances; figure 3 (plan) finally shows the same apparatus adapted for use as a spherometer. The interferometer receives the white light from a collimator at L. After the reflections and transmissions controlled by the mirrors M, M', N, N', and the auxiliary mirror mm', as indicated in the figure, the light is conveyed into the telescope at T for observation of the interferences. The mirror M', is on a micrometer with the screw s normal to its face.

It is through the mirror mm' that the small angles are to be measured and this is therefore mounted at one end of the lever dc, capable of rotating around the long vertical axle aa, in the circular fork FF. The latter is rigidly mounted on the bed of the apparatus by aid of the stem l in the rear. The lever c is bent upward at right angles at d, and it is here that the mirror mm' is firmly



secured by bolts etc., as at *u*. The spring *k* draws the lever toward the front of the diagram, so that the blunt metal pin *e* suitably attached to the end of *mm'* may be kept in contact with the glass plate *g* to be tested.

The plate *g*, in order to be examined as to its degree of plane parallelism, must be capable of sliding up and down or right and left under standard conditions. To obtain these the stout bar *G* rigidly attached like *l* to the base of the apparatus, has been provided, carrying three set screws *h*, *h*, *h*, the points of which lie in the same circumference about 120° apart. They therefore constitute a kind of tripod against which the plate *g* is firmly pressed by the flat spring or clip *rr* and screw *i*. This method of mounting may be appropriately varied in accordance with the tests to be made on the plate *g*, its shape, etc. Similarly the set screws *h*, *h*, *h*, may be placed nearer together or further apart in appropriate screw sockets, and finally the lever *c* may be lengthened or shortened at pleasure. The pin *e* remains in permanent contact with the plate *g* in consequence of a wide circular hole in the clip *rr*; or *e* may clear *rr*, above or below it.

If but one face of the plate *g* is to be tested, the system *Ghrg* must slide as a whole right and left, nearly parallel to the rays *pq*. In such a case everything will depend on the excellence of the slide carrying the system. I did not attempt to make such arrangements, as I had no need of data of this kind; but parts *MM'*, *NN'*, *Fcmm'* and *Grg* were nevertheless mounted on heavy slides (lathe-bed fashion) for convenience in securing a variety of adjustments.

In figure 3 the bar *G* has been reversed in position and the contact pin *e* now passes through a circular hole in *G* to be in contact with a lens *g*, for instance, kept pressed to the tripod screws *h*, *h*, *h*, in the same way as before. The latter should in general be much closer together than the figure shows. The instrument is now a spherometer.

The experiments indicated that the mounting of the contact pin *e* to the extremity of the mirror *mm'* may be the occasion of annoyances. For on sliding *g* right and left or even up and down, the mirror *mm'* is liable to be flexed. In such a case the achromatic fringes rapidly lose sharpness, not to speak of the errors involved. I endeavored to avoid this by keeping the pin *e* out of contact with the plate *g* by a special lever (not shown), while *g* was being displaced and to test a number of successive contacts thereafter; but it is best to mount *e* on a *separate* rigid cross-piece parallel to *mm'* and firmly attached to *c*. In such a case no flexure of *mm'* can occur and the contacts may also be repeated at pleasure. Before each reading the bar *G* should be gently tapped.

The achromatic fringes can be found only through the spectrum fringes. This is not usually difficult remembering that not only must the slit images in the spectrum be in contact throughout, but the two beams must be locally in contact on the mirror *M'*. Moreover the mirrors *M'* and *N'* must be equally thick and the silvered faces all turned towards the auxiliary mirror *mm'*.

In the figures the contact lever *mme* is horizontal. It may also be mounted vertically but in such a case it is less easy to mount the mirrors M, M', N, N', when the system is to be exposed to tremors.

2. *Equations.*—If the mirrors M, M', etc., are set at an angle i, if the deflection of the auxiliary mirror is θ, and if the breadth of the ray parallelogram MM' or NN' is b, we may write

$$b\,\Delta\theta = \Delta N \cos i \tag{1}$$

where ΔN is the displacement at the micrometer at M'. If r is the length of the lever c figure 1 and Δx the displacement of the pin e

$$r\,\Delta\theta = \Delta x \tag{2}$$

Hence

$$\Delta x = (r \cos i / b)\,\Delta N \tag{3}$$

Thus the apparatus is more sensitive as r is smaller and b is larger. In the instrument used (adapted from an earlier apparatus)

$$r = 11 \text{ cm.}; \; b = 10 \text{ cm.}; \; i = 45°$$

so that

$$\Delta x = .778 \Delta N \tag{4}$$

But the main condition of sensitiveness is contained in the size of the fringes, and these may be made indefinitely large by suitable rotation of the mirrors M and M', for instance, in like direction on a horizontal axis (local coincidence of rays on M'). Since

$$2\Delta N \cos i = n\lambda$$

in case of the passage of n fringes, equation (3) becomes

$$\Delta x = nr\lambda/2b \tag{5}$$

Hence the limiting sensitiveness ($n = 10$) would be (with the above data)

$$\Delta x = 33 \times 10^{-6} \text{ cm.} \tag{6}$$

for a single fringe, a few tenths of which may be registered with certainty. When the achromatic fringes are used, it is however usually more convenient to *standardize* the ocular plate micrometer in the telescope, directly by aid of the screw micrometer at M', figure 1. If the ocular plate is divided in tenth millimeters along a centimeter of length and the fringes are of moderate size, one may estimate that about 40 scale parts correspond to $\Delta N = 10^{-3}$ cm. so that a single scale part of displacement of the achromatics is equivalent to $\Delta N = 25 \times 10^{-6}$ cm., while a few tenths of a scale part may here also be estimated.

If the apparatus (fig. 3) is to be used as a spherometer, the ordinary method of measuring from a plate of glass is at once available. If r is the radius of the circle of the tripod and Δx the height of the central foot, we obtain as usual for the radius R required

$$R = r^2/2\Delta x \tag{7}$$

This method gives good results for lenses of all curvatures, however strong, as the tests made indicated. But it is not necessary to use the plate to obtain a fiducial reading, provided the system Gg carrying the lens g, is on good right and left slides. For in figure 4, let θ be the angle between the plane of the tripod and the slides, and let three readings of ΔN be taken for three preferably equidistant points l, c_1 v, of the lens, by sliding Gg over equal distances, r. Let the reading be

$$y = N, \quad y' = N + r\tan\theta + \Delta N, \quad y'' = N + 2r\tan\theta \tag{8}$$

where ΔN corresponds to Δx in figure 4. Hence

$$2y' - (y + y'') = 2\Delta N$$

and equations (4) and (7) apply as before. This method also gives good results even for short distances, r.

3. Observations.—The use of the apparatus figure 1, with the strip of glass g to be tested sliding up or down, did not at first give satisfactory results, because the mirror mm' was too thin (2 mm. thick). It was found however, that on breaking contact at c during the sliding of g between successive positions or by gently tapping the bar or standard G, very fair results were obtainable. There would have been no difficulty in using a thick glass mirror mm' ($\frac{1}{4}$ inch or more) in which case the annoyance of flexure would have been negligible. But all difficulties were eliminated by using an independent arm to carry e, as stated above. Tapping at G before each observation is essential. The observations themselves must be omitted here. Tests of the degree of wedge-shape of long strips of glass from centimeter to centimeter of length, were made in detail, using both the screw and the ocular micrometer. Similarly, lenses of all focal lengths from a few centimeters to 100 cm. either convex or concave, were examined by the apparatus figure 3, with surprising ease and accuracy. The parts of the surfaces of such lenses may be explored to the fraction of a wave length, for successive circular patches of a cm. of radius, or less.

Finally the spherometer method of figure 4 and equation (8) gave entirely satisfactory results. An application for the measurement of (elastic) micrometric displacements will be discussed in a subsequent paper.

INTERFERENTIAL CONTACT LEVER EXPERIMENTS RELATING TO THE ELASTICS OF SMALL BODIES[1]

By Carl Barus

Department of Physics, Brown University

Communicated, December 27, 1918

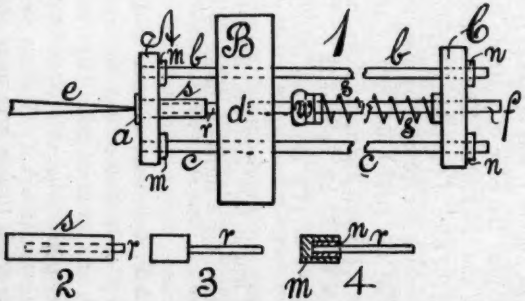
1. Introductory.—In a preceding paper I communicated a series of experiments on the traction modulus of small bodies, using an interferometer design, which worked admirably so far as the optic measurements were concerned. The mechanical part of the contrivance showed an apparent yield, the nature of which I was unable to detect, but which seemed in some way, to be associated with the flexure of parts of this massive apparatus. In fact pulleys and weights were used for imparting stress. It may be argued that any contrivance of this kind, however convenient in other respects, is dangerous because of the force couples introduced, even when the rigid parts of the apparatus are nearly 2 inches thick, as in the case in question.

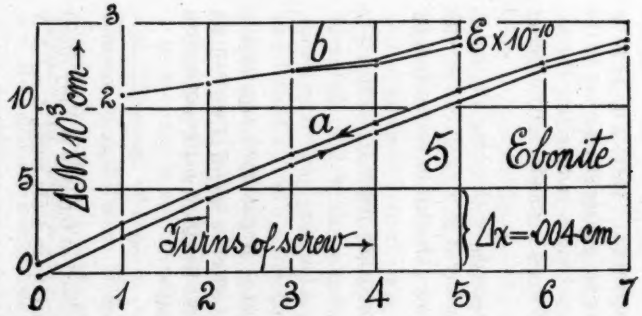
In the present apparatus all this is completely avoided by the use of pushing springs to impart stress, and the interferential contact lever to measure strain. True, friction enters into functioning of such an apparatus to a menacing degree. It thus becomes an experimental question to determine in how far it can also be eliminated by judicious tapping, etc. Cf. § 6.

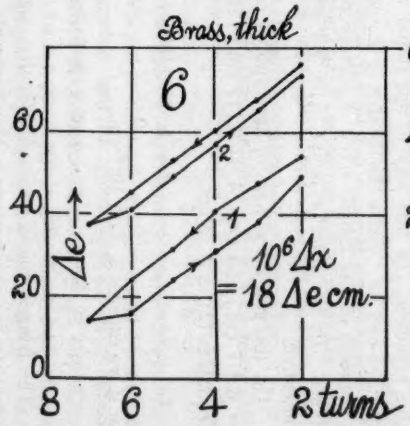
2. Apparatus.—The simplest of the apparatus designed is shown in figure 1. The rod to be tested, 1 to 3 cm. long, is at r held in a brass sheath s, loosely fitting it. See figure 2. This is screwed into the middle of the massive brass cross piece A. A little disc of glass has been attached at a, and the end, e, of the *contact lever* touches it to indicate the small elongations. The longitudinal displacements Δx of the pin e are observed by the interferometer, as explained in the preceding paper.[2]

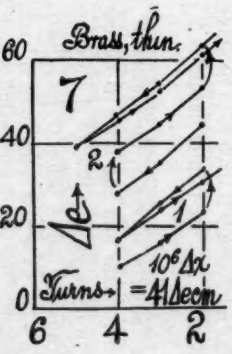
B is a cast iron brick, about 10 inches high, 2 inches thick, and 3.5 inches broad, provided with 2 horizontal $\frac{1}{4}$ inch perforations, parallel to each other and normal to the large face. Through these pass the $\frac{1}{4}$ inch brass rods bb and cc loosely, rigidly connecting the cross piece A with the similarly massive cross piece C (screws and nuts m, n). The rectangle AC is thus free to slide in B, except so far as it is limited by the contact of the rod r with the smooth face of the brick B.

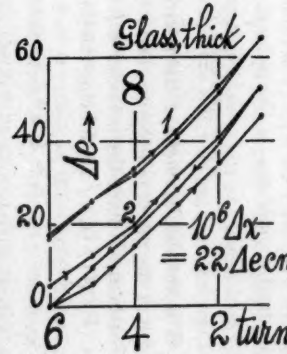
To apply stress, the system d, w, S, f, has been provided, consisting of the stiff open spring S encircling the brass rod df, firmly screwed into the brick B and d, but passing loosely through a perforation in the middle of C. The end near d of the rod df is threaded (20 threads to the inch), so as to admit of the compression of the spring S, by aid of the thumb nut w. S was a precision spring, taken from an indicator apparatus and provided as usual with two end brass collars. It is essential that the sliding parts of the apparatus

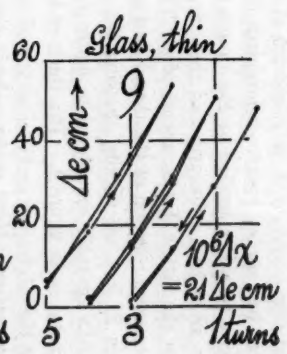

A
B
1
e
a
m
b
s
r
d
c
f
n
2
3
4


b
a
5
Ebonite
Turns of screw
Δx=·004 cm
E x 10⁻¹⁰
ΔE/x 10³ cm
0
1
2
3
4
5
6
7
10
5


Brass, thick
6
Δe
10⁶ Δx
= 18 Δe cm.
60
40
20
0
8
6
4
2 turns


Brass, thin.
7
Δe
10⁶ Δx
= 41 Δe cm
Turns
60
40
20
0
6
4
2


Glass, thick
8
Δe
10⁶ Δx
= 22 Δe cm
60
40
20
0
6
4
2 turns


Glass, thin
9
Δe cm
10⁶ Δx
= 21 Δe cm
60
40
20
0
5
3
1 turns

work smoothly and with a minimum of friction. Such as exists may be eliminated by tapping b and c before each observation. Thrusts up to 15 to 20 kg. may be easily applied by the thumb nut w. These stresses act in the direction ar and fd, colinearly and there are no couples endangering the accuracy of the elastic displacements of r. The stress is standardized in terms of the observed rotation of the thumb nut w.

Figures 2, 3, 4 are details, showing the methods of clutching the rod r, sheathed in figure 2, shouldered in figure 3, soldered in a small cap m with fusible metal n, in figure 4. The thick ends are threaded to be received by A figure 1.

3. Observations. Hard rubber.—As in the preceding paper (l. c.) if Δx is the longitudinal compression of the rod r in s,

$$\Delta x = (r \cos i / b) \Delta N \tag{1}$$

where ΔN is the displacement of the micrometer, at $i = 45°$ to the rays, b the breadth of the ray parallelogram, and r the effective length of the contact lever. Furthermore since the modulus E for the length of rod L and section A is $E = (F / A) / (\Delta x / L)$, F being the thrust,

$$E = F L b / A r \cos i. \Delta N \tag{2}$$

The ocular micrometer if used is to be standardized in terms of ΔN by direct comparison; i.e., if the former datum is Δe arbitrary scale parts, $\Delta e / \Delta N$ must be known.

To graduate the spring S, the apparatus ABC, figure 1, was detached from the interferometer and the brick B fastened near the edge of a strong flat table, with its large face toward A lowermost and horizontal. The rectangle AC was thus vertical, A below C, just clearing the edge of the table. Weights from 1 to 9 kg. were now hung from A, compressing the spring S by measureable amounts. In this way it was found that the stretch 0.7 mm. corresponded to 1 kg. Since the threads of w were 1.275 mm. apart, it follows that 1 rotation of the thumb screw w corresponds to 1.82 kg. or to 1.78×10^6 dynes. In the interferometer, $b = 9.3$ cm., $r = 11.0$ cm. were directly measured.

The test rod r was here of hard rubber of length $L = 2.47$ cm., diameter $= 0.377$ cm., and area $A = 0.112$ cm.^{2} Hence for n turns of the screw w, (equation (2), ΔN in cm.)

$$E = 4.69 \times 10^7 n / \Delta N; \tag{3}$$

or if we express ΔN in 10^{-3} cm.,

$$E = 4.69 \times 10^{10} \times n / \Delta N \tag{4}$$

The fringes, found without difficulty, were small though here more than adequate for the purpose. Measurements were made in *cycles*, care being

taken to repeatedly tap the movable parts of the apparatus before each reading, and these came out remarkably smoothly at once.

An example of experiments made after some improvements of apparatus and carried out with the rod specified through a range of about 15 kg. is given in figure 5, *a*. The rod stood the stress well except at the end where it showed slow viscous shrinkage. The data (ordinates) contain the running displacement ΔN of the micrometer screw in terms of the successive turns of the forcing screw, *w*, figure 1. Stress increasing or decreasing is indicated by arrows.

The values of E were computed from 3 turns (5.46 kg.) of the forcing screw. For loads up to 5 additional turns (total 11 kg.), the data for E are practically identical, both in the outgoing and return series. See figure 5, *b*. At 6 turns (total 13 kg.) the rod apparently yields; but at 7 turns it again stiffens in both cases. As a whole the data are quite as good as the reading of the micrometer screw admits. To interpret this apparent increase of E it would be necessary to use a thinner rod, as the following experiments with brass and glass suggest. Again, only in case of more rigid rods, where ΔN fails, is it necessary to use the ocular micrometer (Δe).

4. The same. Brass.—By way of contrast, a thick solid brass rod, L = 2.34 cm. long, 0.376 cm. in diameter, area A = .111 cm.^{2} was now put into the sheath, *s*, figure 1, and tested, the aim being to redetermine the limit of measurement. Here $n / \Delta e$ or the ocular micrometer is the essential datum and $\Delta e / \Delta N$ must be known.

The interferometer was modified to guard against displacements due to tremor, large fringes were installed and readings were made several times before and after tapping. There was but little difference. An example of such results is given in figure 6, (1) and (2), where $\Delta e / \Delta N \times 10^3 = 43$, and therefore per turn of screw ($n = 1$), $E = 10^{11} \times 19.3\, \Delta e$.

In the first series Δe per turn was 8.0 and hence $E \times 10^{-11} = 2.4$; in the second $\Delta e = 7.8$ and $E \times 10^{-11} = 2.5$. Seeing that a scale part of Δe in figure 6 is but 23×10^{-6} cm., these results are experimentally very good; but their absolute values, as given by E, is nevertheless very low. The rates for the outgoing and return series are identical. The backlash, as it were, on passing from one to the other is probably in the apparatus.

In triplet observations, naturally, higher values of E will be found; for instance, between 3 and 4 turns of the screw, $\Delta e = 5.5$ per turn, appeared in successive independent experiments. Thus $E \times 10^{-11} = 3.5$.

An example may now be adduced of experiments made with a brass rod, thin and shouldered as indicated in figure 3, the large end ($\frac{1}{4}$ inch in diameter) being threaded and screwed into the cross-piece A. The dimensions of the thin part were, length L = 1.8 cm., diameter = .22 cm., A = .038 cm.^{2} The fringe factor was $\Delta e / \Delta N \times 10^3 = 29$. The mean rate per turn was found to be $\Delta e = 4.9$ and $10 \times E^{-11} = 29.2 / 4.9 = 6.0$.

It seemed therefore, worth while to further decrease the section. This was done, the dimensions being, length $L = 1.8$ cm., diameter 0.175 cm., $A = 0.0199$ cm.^{2} The results gave $\Delta e / \Delta N \times 10^3 = 26.0$ and therefore $E = 5 \times 10^{11} / \Delta e$. The rates per turn lie between $\Delta e = 5.6$ (returning) and $\Delta e = 6.1$ (outgoing), so that $E \times 10^{-11}$ is between 8.9 and 8.2, respectively. This is so near the normal value for brass that a further decrease of section of the rod figure 3, was undertaken. The final dimensions were $L = 1.8$, diameter $= 0.138$, $A = 0.015$ cm.^{2} The results are given in figure 7, care having been taken not to overstrain the thin rod. Here $\Delta e / \Delta N \times 10^3 = 25.8$, $E \times 10^{-11} = 66.4 / \Delta e$ and Δe per turn lies between 6.7 (outgoing) and 8.1 (returning). Hence $E \times 10^{-11} = 9.9$ and 8.2, respectively, so that the normal modulus of brass has actually been reached. In fig. 7 the cycles have been spaced as shown by the arrows, to show the separate observations.

5. Glass.—The glass rod first tested was $L = 2.33$ cm. long, 0.37 cm. in diameter, so that $A = 0.107$ cm.^{2}

With a robust interferometer the outgoing and return data were nearly coincident; but the graphs were not as a rule straight. The mean rate per turn was found to be $\Delta e = 8.6$. The fringes were of moderate size ($\Delta e / \Delta N \times 10^3 = 27.5$), so that $10^{-11} E = 12.75 / \Delta e = 1.5$, a very low result. Larger fringes were now installed, giving $\Delta e / \Delta N \times 10^3 = 34.8$. The results after regrinding the contact face are shown in figure 8 and are again nearly coincident, but lie on curved loci. In the first series the larger rate is $\Delta e = 8.4$ per turn; in the second series $\Delta e = 10.0$ per turn. Hence

$$10^{-11} E = 16.1 / \Delta e = 1.9 \text{ and } 1.6, \text{ respectively,}$$

larger than the preceding; but this is still only about one-third of the normal modulus of glass.

The endeavor was now made to proceed as in the case of brass above, with a shouldered rod and thinner sections. With this, in view, the glass rod was fixed in a small hollow cup, figure 4, with fusible metal. The cup being threaded was thereupon screwed into the cross-piece A, figure 1. With a glass rod $L = 1.9$ cm., $A = 0.28$ cm.,^{2} moduli as high as $E \times 10^{-11} = 3$ to 4 were obtained. On taking the rod out however, it became clear that there had been gradual yielding of the fusible metal clutch. Hence I returned finally to the sheath method (fig. 2) using a thin glass rod, $L = 2.54$, cm. long, 0.185 cm. in diameter, $A = 0.026$ cm.^{2} The results are given in figure 9. The graphs are nearly coincident but curved. The mean rates for the higher loads are per turn of the screw, $\Delta e = 10.4$ (incoming) and $\Delta e = 8.6$ (outgoing). $\Delta e / \Delta N \times 10^3 = 28.8$, being the fringe factor, $E \times 10^{-11} = 57.86 / \Delta e = 5.5$ and 6.8, respectively. Hence here also, as in the case of the brass rod above, the normal value of the modulus has been reached; i.e., one may expect the data for E to be correct on their absolute values, if the ratio of length of rod to diameter is of the order of 10 to 1.

6. Conclusion.—The present experiments made with a totally dissimilar apparatus and in a different manner, are nevertheless (notwithstanding the relative simplicity of the present design) not markedly superior to the earlier experiments (l. c.), as a whole. The misgiving which I felt (see § 1) regarding the force couples entering into the earlier method was not therefore justified. Both apparatus function admirably so far as the optics of the method are concerned. This is particularly noteworthy when one considers the admissibility of the rather rough treatment needed in work of the present kind. Both apparatus are liable to give misleading results from the same cause; i.e., from an insufficiently uniform and continuous contact of the two ends of the rod with the abutments. From this results appreciably unequal distribution of stress in the sections of the rod and possibly flexure. There seems to have been no serious yield in the abutments, etc., of either apparatus.

The values of the modules E as a consequence come out too small. There can therefore (tapping admitted) have been no serious discrepancy from friction in the application of stress; for this would have made E too large. Moreover all slight dislocations within the interferometer as the result of any reasonable jar were finally eliminated, so that the cycles practically closed or merely gave evidence of a difference of slope in the outgoing and return series. Such an effect would be expected from viscosity and hysteresis.

I was at first inclined to regard the small values of the modulus E as an actual or trustworthy result, in keeping with the peculiar crushing stress applied. But inasmuch as E may be increased to the normal value by successively decreasing the diameter of the rod, in the case of glass and even of brass, the small values of E must be associated with the lack of contact at the abutments of the rod. Rods about 1 to 2 cm. in length should not be thicker than 1 or 2 millimeters (ratio about 10 to 1), if results are to be assured in their absolute values. And here again a thin rod, r, with *two* thick ends, if both ends are firmly clutched without strain, is the ultimate desideratum. Figures 3, 4, 2 (sheath, s), are admissible expedients, the latter being particularly convenient. The relative results are almost always smooth and admirable to a fraction of a wave length; but for relatively large sections they can not be interpreted owing to the sectional discrepancy in question. This also is relative in its character; at least for moduli markedly above 10^{10}. Thus it is as difficult to obtain the true modulus for a glass rod as for a brass rod, although the latter body is far more rigid.

It is not easy to interpret the apparant hysteresis in many of the above graphs; for this is always associated with possible changes in a complicated train of apparatus. Similarly the different rates in the outgoing and the return series may be variously explained. If the measurements are made in triplets between definite steps of pressure, this difference soon vanishes. Hence this procedure is to be preferred.

[1] Abridged from a forthcoming report to the Carnegie Institution of Washington, D. C.

[2] These PROCEEDINGS, 3, 1917, (693–696).

NATIONAL RESEARCH COUNCIL

MINUTES OF THE MEETING OF THE EXECUTIVE BOARD

AT THE NATIONAL RESEARCH COUNCIL BUILDING,

NOVEMBER 12, 1918, at 9.30 A. M.

Present: Messrs. Cross, Howe, Johnston, Mendenhall, Merriam, Millikan, Pupin, Rous, Washburn, Woods and Woodward.

In the absence of the Chairman of the Board Mr. Woodward, upon motion, served as temporary Chairman.

The minutes of the preceding meeting of October 8, and of the intervening meetings of the Interim Committee, having been circulated by mail, were approved.

The following letter from the President of the National Academy of Sciences, Mr. Walcott, was read for the information of the Executive Board:

> It gives me pleasure to inform you that the plan for International Organization of Research presented by Dr. Hale to the Council of the National Academy of Sciences, and approved by that body at the Annual Meeting of the Academy in April, 1918, has been unanimously adopted by the Inter-Allied Conference, which was held under the auspices of the Royal Society of London, on October 9, 1918.
>
> May I request that you bring this matter to the attention of the members of the Executive Board of the Research Council at its next meeting?

The Acting Chairman of the Council presented the question of the status of the war organization of the Council in view of the fact that the war has been brought practically to an end, and Mr. Pupin

Moved: That a Committee of five be appointed to consider the problems of reorganization of the Council under present conditions, and to make recommendations. *Adopted.*

The Acting Chairman of the Council appointed the following Committee: Messrs. Merriam (Chairman), Howe, Johnston, Millikan, Pupin.

Engineering Division.—Moved: That Mr. George H. Clevenger, of the Bureau of Mines, be appointed Chairman of the Section on Metallurgy of the Division of Engineering, at a salary at the rate of $5,000 per year, the appointment to be during the present fiscal year ending June 30, 1919.

Adopted.

The meeting adjourned at 11.00 a.m.

PAUL BROCKETT, *Assistant Secretary.*

General statements as to recent activities of some of the Divisions of the Council are appended.

Division of Engineering.—The tests of tractors and of special balloons have been very successful, and have established definitely the value of these two devices; and important progress has been made in the experiments on one of the special guns and one type of tank. In addition, a new hydraulic power transmission system, and a new navigation apparatus for aircraft have been taken in hand.

Various committees of the *Section on Metallurgy* have been active and the work of the *Helmet Committee* has been hastened by increasing the experiments in pressing refractory steels into helmet shape. Important progress has been made in the electric welding of ships. An additional committee to study the metallurgical aspects of this welding has been formed.

Division of Chemistry.—The principle item of interest to be reported for this month concerns the investigations in progress at various universities under the auspices of the Division. Fifty Research Problems are now being investigated; three of these have been satisfactorily completed and the data obtained transmitted to the Government agencies needing them.

Division of Geology and Geography.—The text books for use in S. A. T. C. courses, entitled "Military Geology and Topography" and "Introductory Meteorology" appeared toward the close of the month. Several conferences were held under the auspices of the Committee on Education and Special Training on the question of providing maps for use in the S. A. T. C. courses in colleges; as a result of this consultation a special committee has been appointed, of which P. S. Smith is Chairman, to make recommendations.

The Division has taken up the study of the problems of reconstruction in the courses of instruction and research in geology and geography. This subject was referred to the Geology Committee of which J. M. Clarke is Chairman, and to the Committee on Geography of which W. M. Davis is Chairman. Each committee is to consider its particular phase of the problem, including the institution of closer relations between scientific and engineering courses and of adequate provision for pure research.

Major Douglas W. Johnson, a member of the Executive Committee of the Division, has returned from Europe after several months' absence, during which he had exceptional opportunity to study the utilization of geological and geographical information in war. Through his observations and his very clear and concise reports to the Division he has been able to supply information of great value.

Division of Medicine.—During the month of October the Medical Division of the National Research Council received for use in research from Dr. W. W. Keen an additional gift of $101.74 representing further proceeds from the sale of his book entitled "The Treatment of War Wounds." Special disbursements were made in support of work (a) on the cause of influenza, and (b) the employment of "immunized" skin grafts in the healing of infected wounds. Six new research projects were authorized.

Division of Agriculture.—Considerable time has been given to the preparation of courses for the Students' Army Training Corps, in Biology, Animals in Relation to Disease, Protozoa, and Economic Botany, and of a Wood Inspectors' course with a view to the preparation of men competent to inspect wood for various uses.

The work of the *Fertilizer Committee* is progressing satisfactorily. Plans have been made with the coöperation of the U. S. Department of Agriculture to enlarge the project to include several additional states in the Mississippi Valley. Preliminary steps have also been taken to organize group projects in the eastern and southern states with reference to the use of fertilizers.

A *Salt Nutrition* project, which has for its object the determination of the absolute salt requirements, with special reference to potash, of important crops, has been formulated and coöperative arrangements completed for the inauguration of the work.

Considerable attention has been given by the *Committee on Botanical Raw Products* to sources of fiber supply, rubber and various valuable gums and resins, and reports regarding these projects have been furnished to branches of the Government where work seemed to be most closely related.

The work of the *War Emergency Board of Plant Pathologists* has progressed very satisfactorily and several important circulars have been issued by the Board for the information of plant pathologists in this and foreign countries. The losses to our food crops through plant diseases are enormous and work of this kind offers one of the best means for increasing the food supply. The Chairman of the Division is in Europe on special business for the Government and while there will confer with the foreign representatives of the Council and with representatives of some of the European Governments regarding coöperative work in the control of plant diseases. This is especially important with reference to potato diseases as this crop must be depended upon for supplying a large part of the starchy food for Europe as well as for our own army.